

## ***Interactive comment on “Modelling dominant runoff production processes at the micro-scale – a GIS-based and a statistical approach” by C. Müller et al.***

**C. Müller et al.**

Received and published: 9 September 2008

The authors would like to thank the anonymous referee #2 for the comments.

### GENERAL COMMENTS

Review Comment: Nevertheless, I would suggest to omit the short versions of the geological units in the brackets because these abbreviations will neither be used in the future nor will they appear in any map. It might be useful to show the spatial distribution of the geology of the basin in a map as the geology will be important for describing and analyzing the results.

**Author Comment:** The authors will skip the short version of the geological units and insert a map of the spatial distribution of geology of the basin. Since an additional meso-scale basin will be included in this study for validation purposes (see also general author comment) a geological map of this area will be included as well.

**Review Comment:** In the methodology chapter, the procedures of both approaches are explained clearly, additionally a short description of the Canonical Discriminant Analysis (CDA) would be helpful.

**Author Comment:** Canonical Discriminant Analysis (CDA) is a statistical method to generate linear regression models for variable reduction and to obtain with them the explanation for an experimental design grouping the data into classes. The authors will include a short description with references to basic literature and common applications in natural sciences.

**Review Comment:** I also suggest to change the heading of the chapters 3.1 and 3.2. It would be coherent for the reader of the paper, if you entitled the two approaches by using specific names like GIS-based approach or statistical-based approach.

**Author Comment:** Specific names for the GIS-based approach and the statistical-based approach will be used and incorporated in the revised manuscript.

**Review Comment:** The application of both approaches provides satisfying results which are described and discussed in chapter 4. Unfortunately, a critical discussion by setting relations to other similar research studies is missing. Only a comparison to Scherrer and Naef (2003) is given. Especially at page 1685, line 12 to 17, I would expect some similar references. I think the result is right but it should be explained more precisely.

**Author Comment:** The aim of the study was to develop to different approaches, which simplify the complex method of Scherrer and Naef (2003). The results of the two approaches presented in this study should resemble both in terms of method and ref-

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erence truth the original Scherer & Naef (2003) approach (see also answer to RC S893). Concerning page 1685, line 12 to 17; the part typical low mountain range will be removed from the text.

Review Comment: At page 1687, line 5-16 there is only a repetition of correctly or incorrectly classified areas without analysing the reasons for these discrepancies.

Author Comment: The authors agree and will adapt the data description to crucial observation and deepen the explanation.

Review Comment: Also I would expect three maps with generated DRP, two based on the new approaches and one based on the studies of Schobel (2005).

Author Comment: A third map will be added, which gives the results of the statistical approach.

Review Comment: The next step could be to model the runoff based on the three maps and then to make a decision what is the best approach for generating DRP.

Author Comment: It was not the intention of the authors to model rainfall runoff relationships and this will be stated more clearly in the manuscript (see also general author comment). Furthermore, the authors would like to state that the use of the DRP maps in hydrological rainfall runoff models is object of further study. This will be discussed in the revised version of the manuscript.

Review Comment: In chapter 4.1 (page 1685, line 4), there is one point which is very important for the whole paper. The conclusion is drawn, that the resolution of the DEM is not perfect but the whole statistical approach is based on this DEM because the geomorphometrical features are derived from this DEM. This could be another reason for wide differences between the GIS-based generated DSOF1 areas and DSOF1 areas in the map of Schobel (2005). All the convergent slopes in the middle of the sub-basin Grundgraben are not included. I would suggest at first to improve the DEM afterwards further analysis could be made.

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Author Comment: The authors agree that the resolution of the DEM is important. However, the differences between the GIS based generated DSOF1 areas and the DSOF1 areas in the map of Schobel (2003) are only conditionally based by the resolution of the DEM. More than that, part of the Scherrer and Naef (2003) methodology is the application of aerial photography and topographical maps. In combination with an intensive field campaign, this offers the possibility for the implementer of the original method to delineate exactly the DSOF1 areas in the riparian zone, far better than any approach using even a detailed DEM. This will be stated more clearly in the revised manuscript.

Review Comment: In the end the authors have drawn some conclusions but there is no final comment which approach is to prefer or can be recommended for the regionalization in the meso-scale.

Author Comment: The authors will address this point in the conclusion of the revised manuscript.

Review Comment: At page 1688, line 21 the authors propound that soils and soil toposequences reveal a strong correlation to the spatial distribution of the DRPs but no analyses about these dependencies have been described in the previous chapters.

Author Comment: This conclusion will be withdrawn.

## SPECIFIC COMMENTS

Review Comment: Title: Do not use the word modelling because with the two approaches you simply identify hydrological response units.

Authors Comment: The title will be adapted (see also general author comment).

Review Comment: Page 1678, line 18: Change Scherrer et al. 2006 into Scherrer 2006

Author Comment: This will be adapted in the revised manuscript.

Review Comment: Page 1678-1679: The introduction gives a good short overview

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about process studies in the micro and the hillslope scale. But I miss a proper overview to other studies, which present methods for identifying hydrological response units. There is a concentration on the results of Scherrer, Naef and Schmocker-Fackel who worked at the same institute at the ETH of Zuerich.

Author Comment: The authors will adapt the introduction and provide an overview to other studies, which present methods for identifying hydrological response units. On the other hand, the authors used this approach as reference for a specific type of soil functional mapping with intense data input. It was not the intention of the authors to evaluate the suitability or accuracy of the methods developed by the team at the ETH of Zürich.

Review Comment: Page 1680, line 24: The geological abbreviations i.e. so1 or so2 can be left out.

Author Comment: See above.

Review Comment: Page 1681, Line 1: What means surface gley?

Author Comment: This will be removed from the manuscript.

Review Comment: Page 1681, Line 5: Some information about the runoff would be good.

Author Comment: Unfortunately, no runoff data with a sufficient duration and quality is available for the Zemmer basin. See also general author comments.

Review Comment: Page 1681, line 13: Please omit the word see in the Figure and Table announcements.

Author Comment: This will be adapted in the revised manuscript.

Review Comment: Page 1681, Line 14: Do you mean 16 rainfall simulations or 16 rainfall-runoff simulations? Because with rainfall simulations it is not possible to determine hydrological response units.

Author Comment: 16 sprinkling experiments have been conducted at the Zemmer basin. However, they were not used directly to determine DRP. Therefore, this part will be removed from the manuscript.

Review Comment: Page 1681, Line 16: Mueller 2008 is not listed in the references.

Author Comment: The reference will be removed from the text.

Review Comment: Page 1681, Line 16: Change Mueller 2007 into Mueller et al. 2007.

Author Comment: This will be adapted in the revised manuscript.

Review Comment: Page 1682: In general what is the difference between impermeable and permeable? Does this classification base on the geological or on the pedological information? If it is based on the soils then I would not agree that there are only two classifications. If it is based on the geology then I note to take the soil characteristics more into account.

Author Comment: The classification is based on the assessment of the permeability as suggested by Zumstein et al. (1989), who classified the infiltration permeability of the substratum with respect to its lithology and geo-hydrological characteristics such as fractures and porosity obtaining eight different permeability classes. The classification of Zumstein et al. (1989) was adapted and simplified into only two classes: permeable and impermeable. Concerning the soil characteristics, however the objective of the study is to derive DRP maps with a view to regionalization to areas where this soil information is lacking. Therefore, the authors choose to refrain from taking soil characteristics into account. This is also the major topic in which this study differs from Scherrer and Naef (2003) and Schmocker-Fackel et al. (2007).

Review Comment: Page 1682, Line 1: What means PBS 2006? This reference is not included in the reference list

Author Comment: PBS means in German Prozessbeurteilungsschema. It is the original decision tree for field campaigns to determine DRP according to Scherrer and Naef

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(2003). The reference is Scherrer (2006). It will be adapted in the revised manuscript.

Review Comment: Page 1682, Line 16: The result of the first approach is not a model, only a map with the DRP is generated by crossing different information layers.

Author Comment: The word model will be used more carefully (see also general author comment).

Review Comment: Page 1686, line 12: What are the reasons to choose this parameters?

Author Comment: Parameter inclusion into the CDA followed the criteria mentioned on page 1684, lines 2-4. This is the case for a stepwise CDA with parameter exclusion. The additional advantage of this method is that it allows identifying relevant parameters for the grouping into classes. As mentioned above, the authors elaborate the description of the CDA and explain the method in more detail.

Review Comment: Page 1686, line 19-29: In general what is the difference between SOF1, 2 and 3 especially what is the meaning of the number? Please explain.

Author Comment: The number reflects, according to Scherrer and Naef (2003), the velocity of the process activation during a rainfall event. DSOF1 is a fast saturation overland flow whereas DSOF3 is a slow saturation overland flow. This means that the process starts after a larger amount of rainfall. A short description of the method will be given in the methodology chapter.

Review Comment: Page 1702: There is only one map shown but there are two approaches. I would expect two maps additionally to the map of Schobel (2005).

Author Comment: A third map will be added, which gives the results of the second approach (see also above).

Review Comment: Page 1694, Table 3: Several classes of risk are listed in the table, but in the text there is no explanation how these classes are defined. Although there

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is a short description in the legend but it is not clear how the boundaries between the different classes are derived.

Author Comment: The authors agree with the referee statement and will adapt this in the revised manuscript.

## TECHNICAL COMMENTS

Review Comment: Figure 1: Please display also the stream network in the map. In the legend the sign for the boundaries of the catchment and the sub-catchments are missed.

Review Comment: Figure 5a. Please revise the axes labels and use a bigger font size.

Review Comment: Table 5: The legend is very confusing. Perhaps the names of the variables could be changed. Please explain the values of the canonical function coefficients in a short sentence.

Review Comment: Table 6: I think the sign for percentage is at the wrong place.

Review Comment: Table 6: It is confusing that the upper matrix is in ha and the lower matrix is in percent.

Author Comment: All technical comments are justified and will be implemented in the manuscript.

## FINAL OPINION

Review Comment: In my opinion the objective of this paper is of international interest but it has to be revised basically before re-submitting. I would suggest to improve the discussion basically especially take more similar references into account. What is the difference to other concepts which also identify hydrological response units and especially what are the new advantages of your approaches. There are several quite good research studies concerning to this subject like Fluegel 1995, Tilch et al. 2006, Tilch et al. 2002.

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Author Comment: The authors will discuss the different approaches more intensively and explain the differences to other studies more in detail.

Review Comment: Please make a final decision and recommendation which approach is to prefer or can be recommended for the regionalization in the meso-scale. Also the validation of the three approaches by runoff modelling should be done.

Author Comment: Since a second basin will be incorporated in the study, the authors will discuss which of the two approaches is better suited for regionalization purposes. The results of the original method of Scherrer and Naef (2003) (i.e. maps with dominant runoff processes) are not suited to predict discharge. The in this study presented approaches simplified the method of Scherrer and Naef (2003) in such a way that it now can be applied in micro-scale basins without using the heavy data load, which was necessary in the original method. The in this study presented approaches are not able to be used directly in discharge prediction, yet.

Review Comment: At least please ask for a native English person to edit the paper there are some mistakes.

Author Comment: A native English speaker will check the entire manuscript.

## References

Scherrer, S. and Naef, F.: A decision scheme to identify dominant flow processes at the plot-scale for the evaluation of contributing areas at the catchments-scale, *Hydrol. Process.*, 17(2), 391-401, 2003.

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Grundgrabens und Schleidweiler Baches bei der Gemeinde Zemmer/Eifel: Ber. f. d. Forschungsanstalt für Waldökologie und Forstwirtschaft Rheinland-Pfalz, Trippstadt, 2005.

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